Separate and Unequal in the Labor Market: Human Capital and the Jim Crow Wage Gap

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Abstract

We decompose the 1940 black-white earnings gap into that part attributable to differences in human capital and an unexplained portion that traces the upper bound of labor market discrimination. We find that differences in measurable human capital play a predominant role in determining 1940 wage and occupational status gaps. Our range of estimates for the unexplained gap, 11 to 17 log points, coincides with the higher end of the range of estimates from the post-Civil Rights era. We estimate that a counterfactual "separate but equal" school quality standard would have reduced wage inequalities by as much as 52 percent.

1 Introduction

The American labor market has long exhibited a sizable gap in wages awarded to black and white workers, motivating a large body of research devoted to disentangling the role of human capital, or "pre-market," factors from more structural labor market issues and, chiefly,

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wage discrimination.[1] This decomposition exercise has important policy implications. If pre-market skill gaps are largely responsible for pay differences, appropriate policy solutions should prioritize disparities in human capital accumulation over direct labor market interventions. At the same time, if residual gaps unexplained by measurable human capital can be confidently labeled discrimination, direct labor market interventions are also apt.

Detailed human capital measures are of tantamount importance for this exercise. As a result, existing literature on the conditional wage gap largely focuses on the post-Civil Rights era where data on individual human capital and earnings are much richer. In this paper, we use recent data advances to extend the time series of wage decomposition to the 1940 U.S. Census. Census respondents' educational attainment values have long been available in public use samples of the 1940 census, but assessments of the quality of time in school have, to date, been limited to state-level summary statistics. [2] But these state averages mask large intraracial variances in the value of school resources, and we show that aggregated measures of school quality lead to misleading conclusions about the contribution of school quality to labor market outcomes.

We develop a new panel of county-by-race school quality statistics for each year between 1920 and 1940 for ten Southern states in order to assign young men in the 1940 public use microsample (Ruggles et al., 2010) a school quality metric specific to their race, age, and probable county of education. In doing so, we rely on newly released data describing county of residence and recent mobility as of the 1940 census. In addition to years of schooling and the quality of available schooling, we utilize a known oddity in the World War II enlistment records to impute Army General Classification Test (AGCT) scores for Southern males in 1940 as a third measure of human capital comparable to Armed Forces

[1] See Lazear (1991); Oettinger (1996); Darity and Mason (1998); Altonji & Pierret (2001); Lang & Manove (2011). Also see Lang & Lehmann (2012) for a more complete survey of the racial discrimination literature.

[2] In the 1930 South, according to public reports of state education departments, annual spending per enrolled black pupil was typically \$9, versus \$61 in spending per white pupil. (Authors' calculations using county-level school resource data described in Section 3). Although school quality in the post-Reconstruction South was relatively similar across races, and although *Plessy v. Ferguson*, 163 U.S. 537 (1896), insisted on "separate but equal" schooling, the interwar years witnessed large increases in spending on white schools relative to black.

Qualification Test (AFQT) scores utilized in more recent work. With this battery of human capital metrics, we estimate the proportion of black-white labor market outcomes attributable to human capital and that which remains unexplained. We focus on the southern states because the vast majority of black males resided in the South in 1940 and because regional wage gaps were still large in the early years of the Great Migration.

We find that pre-market human capital disparities are the predominant determinant of racial gaps in 1940 for a number of measured labor market outcomes. For young, employed males in our sample, the age-adjusted difference between black and white wages in 1940 was a substantial 47.7 log points on an annual basis and 49.6 log points per week worked. These gaps attenuate to 11.3 and 17.1 log points, respectively, when we condition on educational attainment and school quality. Notably, these estimates are at the high end of the range of conditional wage gap estimates in more recent data. Differences in educational attainment account for roughly half of the attenuation while differences in the quality of schooling account for the remainder. It does not appear that school quality measures are simply proxying for local labor market discrimination. School quality *per se* had little effect on wages, but rather, served to enhance returns to years of schooling.[3] When we impute standardized test scores for our sample, the conditional weekly wage gap falls to 10-12 log points while the annual wage gap is statistically insignificant. At the same time, and echoing the previous literature, we find that conditional wage gaps within occupations were smaller or statistically indistinguishable from zero (4.1 log points annually and 12.8 log points per week worked). Occupational sorting by race was one barrier to higher wages, and our measures of human capital explain no more than half of the racial gap in an index of occupational standing. Still, our wage gap estimates indicate that occupational sorting served to separate black and white wages by a wedge only slightly larger than that observed after 1970.

Finally, we evaluate the role of separate and unequal schooling for blacks and whites in the wage gap by simulating a "separate but equal" mandate within a binding budget constraint to equalize school quality within counties Because blacks were disproportionately

[3] See Section 5 for additional evidence in this vein.

located in areas with lower overall education budgets, the mandate only partially closes the racial gap in average school quality. Still, the wage gap falls by 28-42 percent. When we further allow educational attainment to be endogenous to school quality, the wage gap is reduced by up to 52 percent.

Like others before us, we interpret conditional black-white labor market gaps with care, recognizing that they are consistent with both racial discrimination and omitted variables (Lang & Manove, 2011). To the extent that bias in this measure of discrimination is consistent over time, however, our findings imply that discrimination was only somewhat more crippling for racial wage equality in 1940 than it was much later, when equal employment protections were in place. We conclude that the discriminatory preferences of white Southerners were powerful in limiting black public school quality and reducing the wages of blacks through the human capital channel, but were far less powerful in affecting wages through labor market discrimination. This does not rule out a role for employment policy interventions later on, but implicates the public sector as the responsible party for the bulk of Southern wage disparities in the Jim Crow era.

2 Literature on the Black-White Wage Gap

Ex ante, one might expect the unexplained portion of wage differentials to be greater in 1940 than that observed later in the century. When driven by discrimination, racially separable wage equilibria depend on the number and size of discriminatory firms relative to the group being discriminated against. Further, the size of the wage gap is a function of the disutility of employing workers in this group. Thus the gap is increasing in the prejudicial preferences of the general population because it both increases the number of discriminatory employers and the disutility of employing black workers.[4] In the period in question, extensive discrimination was evident in racially segregated job listings and stark differences in publicly-reported salaries for black and white teachers.[5] Further, our analysis

[4]These implications are true both in Becker's (1957) original framework and in adaptations to a search model as discussed in Lang & Lehmann (2012).

[5]See Goldin (1990). Across ten Southern states in 1930, white (black) teacher salaries averaged \$5.89 (\$2.55) per day in session. See Section 3 for sources and Margo (1984) for further discussion.

pre-dates the Civil Rights Act of 1964 and the associated employment and pay protections that outlawed labor discrimination against black Americans. Finally, evidence that racially discriminatory views in the United States have declined over time has given rise to the idea that discrimination plays a smaller role in the black-white wage gap than it once did (Fryer, 2011).[6]

Still, economic historians have many times failed to detect racial wage discrimination in the early part of the 20^{th} century, observing that black and white workers received equal pay within particular occupations, even in the U.S. South, and implicating the relative human capital of black Americans for their low relative wages.[7] But in concentrating on micro data from particular occupations and firms and in the inability to observe relevant human capital measures, this literature has been less successful at determining whether black and white workers across skill levels and occupations received equal wages conditional on human capital.[8] The empirical validity of equal pay for equal *human capital* remains unknown.[9] Our data allow us to measure pay gaps on multiple intensive margins of employer-employee interactions: annual wages, weekly wages, occupational sorting, weeks worked, and wages conditional on occupation.

For the modern era, Table 1 presents a limited review of related papers measuring the contributions of schooling, school quality, experience, ability, and family background to the overall wage gap. The second column of the table lists the data source and cohort used in each analysis. The third indicates which human capital variables are included in the study and the fourth indicates what percentage of the overall gap they explain. The fifth column reports the log conditional black-white gap. The overwhelming indication from

[6]See Lang & Lehmann (2012), Figure 3, for evidence of a decline in prejudice measures after 1956. The decline continues through the racially charged 1960s. To our knowledge, no data for 1940 exist, and no regionally subdivided data are available either.

[7]This observation is particularly consistent prior to the 1920s (Fishback, 1989; Smith, 1984; Smith & Welch, 1989). Whatley & Wright (1994) and Wright (2013) cite evidence of a more substantial wage differential for entry-level workers by 1937, a difference they attribute at least in part to a yawning racial gap in human capital related to schooling.

[8]Alternatively, "(un)equal rewards to otherwise identical workers" (Whatley & Wright, 1994).

[9]Others research indicates that human capital attainment is endogenous to labor market opportunities, including human capital. Still, there are other determinants. For example, Aaronson & Mazumder (2011) show that exogenous changes in school quality induce changes in educational attainment in this era for black southerners.

this literature is that pre-market factors matter for determining wage differences, and, in many cases, the wage gap potentially attributable to labor market discrimination is minimal after controlling for these factors. Estimates from later decades of the 20th century have the advantage of access not only to measures of schooling and school quality but also scores from standardized tests taken while the individuals are still in school. Even with this additional measure of ability, modern estimates of the wage gap find a strong link between school quality and academic achievement (Lang & Manove, 2011).

Despite a wealth of late-century research on components of the wage gap, much of it highlighted in Table 1, it is not obvious *a priori* that the school quality gap should have a similar bearing on the wage gap in 1940. Margo (1986), Welch (1974), Smith (1984), and Smith & Welch (1986, 1989) represent the earliest wave of research highlighting the rising quality of black education as an important driver of gains in the black-white earnings ratio, but to date, direct tests of the impact of improving black school quality have relied on state-level school data or focused on a time period after employment protections were in place (Link & Ratledge, 1975; Link et al., 1976; Nechyba, 1990; Card & Krueger, 1992a,b; Ashenfelter et al., 2006). Notably, Card & Krueger (1992b) find a differential return to schooling across blacks and whites in the census which, in turn, can be attributed directly to differences in state-level school quality metrics. They conclude that 20 percent of the narrowing of the black/white earnings gap between cohorts born in the 1920s and those born in the 1940s (measured between 1960 and 1980, in the midst and wake of Civil Rights) is attributable to rising school quality.[10] But wage gains for black Americans sharply deviated from underlying trends in the 1960s, suggesting that the federal antidiscrimination policies of that time were more important than rising human capital (Heckman & Payner, 1989; Donohue & Heckman, 1991). Thus, the extent to which human capital

[10]Several others have quantified the impact of school quality on earnings, *per se*, without a particular focus on the black-white gap. For our period, the most relevant of these are Johnson & Stafford (1973), Morgan & Sirageldin (1968), and Morgenstern (1973). Each use state-level data on school quality. Wachtel (1975) and Wachtel (1976) document positive returns to school quality for a selected sample of individuals likely restricted only to whites. See Betts (2010) for a summary of the literature on the effect of school quality on earnings. See also Rizzuto & Wachtel (1980) for an estimate of the social rate of return to investments in school quality for whites and blacks separately in the 1960 and 1970 census.

proxies mattered in 1940 is an empirical question.

Finally, we note that a consistent feature of the literature is that estimated returns to school quality differ depending on whether data are at the state, district, or school level. *Ex ante*, a primary limitation of state-level approaches would seem to be aggregation (attenuation) bias discussed explicitly in Morgenstern (1973). More granular school quality indices, however, do not necessarily increase the estimated returns to schooling (Betts, 2010).[11] Further, the possibility of non-linearities in the returns to school quality imply that assigning individuals the average level of school quality for the state in which they were educated will generate a bias in the estimated returns, although the direction of bias is ambiguous. In the Online Appendix, we document conditional wage gaps using state level data that deviate from our baseline estimates by up to 20 log points. These deviations represent as much as a 200% adjustment to the coefficients of interest and justify our view that more granular school quality data are necessary to adequately profile human capital.

3 Data

The public-use sample of the 1940 U.S. Census (Ruggles et al., 2010) contains one of the earliest available micro-level data on wages for a cross-section of the U.S. population.[12] Prior to 1940, labor market measures in census returns include occupation and industry of employment, but no individual earnings data.

1940 census enumerators recorded labor market wages but not non-wage income. Consequently, the self-employed (including a substantial number of farmers and farm tenants) do not report income in this sample. As such, we exclude individuals without recorded earnings from our main results.[13] Occupational score results are robust to including some

[11] "Most of the studies that find no link or a weak link between school inputs and student outcomes measure school inputs at the level of the actual school attended; studies that do find a strong effect typically measure school resources at the level of the state."

[12] There are precious few sources for labor market earnings other than the Census prior to the advent of the National Longitudinal Survey of Youth. Notable exceptions are the 1915 Iowa census (Goldin & Katz, 2000), where perhaps 1 percent of respondents were black, and the NBER Thorndike-Hagen sample exploited by Wachtel 1975; 1976, where the sample is believed to be limited to white males.

[13] Farm laborers remain in the sample. If labor market discrimination emerges either from the tastes of employers or co-workers, unobserved self-employed farmers are unlikely to have experienced such wage

of these individuals, as we show in supplemental analyses discussed in the Appendix.[14]

In addition to earnings measures in the census, we generate an occupational score for each individual in the sample based on their 1940 reported occupation. Our constructed occupation score variable is the average 1950 wage among white males for each reported occupation in the 1940 manuscripts, mapped to a three-digit code.[15] The occupational score is best thought of as an index of occupational standing that is comparable across races, ages, genders, and geographies, abstracting away from race-dependent occupational or regional sorting. Keeping this in mind, we are less interested in the score's cardinal properties, which are affected by the Great Compression leading up to 1950 (Goldin & Margo, 1992), than its relative attenuation once we condition on human capital.

The 1940 census contains measures of the highest grade completed by each individual. Census enumerators were instructed to record the "highest full grade that the person has successfully finished." Despite this instruction, there is some concern that the question was interpreted differently across races, especially for (predominantly older) black individuals who were educated in ungraded schools (Margo, 1986). In that case, the census instructions directed enumerators to record the number of years the person was in school.[16] Our focus on younger men in the data avoids much of this form of mismeasurement.[17] We identify

discrimination, and our conditional wage gap estimates are biased upwards. We acknowledge that we are not modeling selection into wage-earning occupations, although the Appendix illustrates that human capital has a substantial effect on the likelihood of farming employment. Elsewhere, the literature has identified larger wage gaps for skilled occupations (Wright, 2013), thus, excluding lower-skilled farmers from the analysis may overstate the underlying black-white earnings gap..

[14]Another drawback of the 1940 census is that we cannot adequately measure payments-in-kind as part of wages. This limitation is problematic given that there is substantial agricultural employment in this time period, even after omitting farm owners and tenants without reported earnings. Payments-in-kind may have been more prevalent for agricultural workers, and in particular, for black agricultural workers. but robustness checks described in the appendix show that findings are insensitive to the exclusion of respondents with more than $50 in non-wage income, which includes payments in-kind as well as interest income and self-employment income.

[15]Again, we rely on Ruggles et al. (2010). We do not use the occscore variable contained in their data because it is calculated using all workers, regardless of race, and thus captures wage discrimination.

[16]"[I]f this cannot readily be determined, [enter] the number of years the persons attended school."

[17]To the extent that there remains overstatement of "highest full grade" for men with an ungraded education (perhaps because grades were typically completed in more than one year), it will serve to bias *downwards* the contribution of differences in human capital to the black-white earnings gap and overstate the role of labor market discrimination. See Margo (1986) for evidence that using respondents' highest grade completed as a proxy for educational attainment understates the contribution of human capital to the 1930-1970 decline in the wage gap.

working men aged 18 to 25 from the public-use sample who reside in one of ten Southern states for which we have education quality data (described below): Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Texas. The age controls are designed to ensure that we can more accurately assign individuals to their county of schooling and reasonably ignore differential on-the-job training or experience.[18]

To measure the quality of schools available to each individual in our sample, we utilize transcribed county-level measures of race-specific school resources in the years leading up to 1940 for these ten Southern states. Over much of the 20^{th} century, each U.S. state's department of education or equivalent office published an annual or biennial report containing statistics on revenues and expenditures, disaggregated by county and by race. With the exception of a small number of biennial editions, these education reports allow us to measure at least one race-specific school quality statistic for each year for each county. The data and data collection process are described more fully in the Appendix.

The school quality data can be matched to each individual in the Census data after making some assumptions about where individuals lived when they were young. In 1940, census takers inquired as to the location of respondents five years prior, in 1935. This detailed geographic information is newly released in the microsample files. We assume this 1935 location is the county of residence during an individual's potential schooling years. Because our analysis focuses on individuals 25 and under in 1940, this amounts to assuming that individuals aged 13 to 20 in 1935 reside in their county of schooling.[19]

[18]We cannot calculate labor market experience as the difference between age and years of schooling (plus 6 or 7) because age-in-grade differed significantly between black and white students. (Collins & Margo, 2006)

[19]We note that the 1930s were a decade of extremely low internal migration in the United States. To gauge the mismeasurement in this assignment, we look to a sample of death records from North Carolina generously shared with us by John Parman. The records include both county of birth and county of death for deaths reported prior to 1976. We examine a subset of males who died between the ages of 7 and 20 and were born in the relevant years (1914 to 1923). For these individuals, we find that 30% lived in a state other than their home state by age 7 as indicated on their death certificate and 38% did the same by age 20. (The numbers are 26% and 42% using a 3-year moving average.) Thus, by assuming that an individual observed at age 13-20 in 1935 lived in the same location at age 7 when they would have entered school, our methodology will falsely identify the county of education for up to 8% of individuals in the sample (16% using the moving average). The unfortunate assumption in this, and the only one we can reasonably make, is that individuals only move once so that the number who have relocated from their birth county by age 20 less the number who did the same by age 7 captures all migration. Note also that we undertake sensitivity tests on

The ability to identify the probable county of education for individuals allows us to assign a race-specific school quality measure more proximate to the actual education experience of individuals than previously possible.

Depending on the year and state, school quality data are comprised of one or more of the following eight metrics: expenditures per enrolled pupil, expenditures per pupil in average daily attendance (ADA), teachers per enrolled pupil, teachers per pupil in ADA, certified teachers per enrolled pupil, certified teachers per pupil in ADA, term length, and average teacher salary. Reported measures vary by state and year, but definitions are consistent across races within a state-year (and county-year). States had some leeway in the metrics they chose to document, and many changed the format and set of data reported over time. But, importantly, if a white-specific metric is reported in a given year, a corresponding black-specific metric is as well. An inventory of the school quality statistics available for each state and year can be found in the Appendix.

Selecting a single metric to proxy overall school quality is an untenable strategy. Each metric represents a different view of school resources, and more practically, varying availability of data across states within years and across years within states limits the scope of a given metric. Instead, for each quality measure, we calculate a within-year Z-score relative to all other counties in the data who report the same quality metric. The index computation is as follows

$$Z_{jct} = \frac{M_{jctr} - \bar{M}_{jt}}{\sigma_{jt}}$$

where M_{jct} is the value of metric j (e.g., teachers per enrolled pupil) in county c in year t for race r. $\bar{M}_{jt}$ and σ_{jt} are the mean and standard deviation of measure j across all county-race observations reporting the same metric in year t. We emphasize that the conversion is relative to all county years reporting the same statistic and is across, not within, race.[20] Z_{jct} converts statistic M_{jct} to a scale with mean zero and unit standard deviation that can be compared across counties and races in year t. In state-years where more than one measure

the county-of-schooling assumption in the appendix, finding little change in wage or occupation score gaps when we restrict the sample to respondents whose state of birth matches their 1935 state of residence.

[20] See the appendix for results using pooled Z-scores across counties, races *and* cohorts.

of school quality are reported, we use the average value of Z_{jct} across all available $j's$.

$$Q_{ct} = \sum_{j=1}^{J} Z_{jct}/J$$

where J is the total number of available metrics for county c in year t.[21] The exception is when a single metric is available with both enrolled pupils and pupils in ADA as a denominator. In this case, we use only the measure per enrolled pupil.

The index achieves two goals. First, it allows us to aggregate information about school quality across quality metrics that differ in their distribution and coverage.[22] Second, by calculating a within-year Z-score, we reduce the influence of cyclical variation on school quality measures. This is especially important given variation in school funding over time in the 1930s attributable to changes in the macroeconomic environment. Q_{ct} school quality measures are relative; the measure for each county-by-race cell is standardized relative to all others in the same year.[23]

The education panel and 1940 individual-level labor market data are merged so that school quality measures can be inferred for each individual, given our assumption about their 1935 location. In order to assign school quality measures to individuals, we must make a final assumption. We observe individuals' ages and years of schooling in 1940, but not the precise years of enrollment. We assume that all individuals are "at risk" for school enrollment between the ages of 7 and 18 and measure average school quality across those

[21] Robustness checks described in the appendix control for the quantity of school data: i.e., J and J^2. Additionally, results are not sensitive to the inclusion of eight controls measuring the frequency with which each metric was reported during an individual's potential years of schooling (again presented in the appendix).

[22] In an unreported analysis, we decompose the Z-score to understand the relative contributions of its component pieces. We focus on four metrics: expenditures per pupil, term lengths, teacher-student ratios, and average teacher salaries. This limits the analytic sample to Alabama, Georgia, Louisiana, and South Carolina (race-specific total expenditure data are not available for other states). Results indicate that all four quality measures are jointly significant in determining wages, but that teacher salaries and term lengths are most important. When we regress our Z-score index on these two school quality metrics for all county-years in which they are available (which includes the vast majority of the analytical sample of wage-earners), the resulting R-squared statistic is 0.91, implying our Z-score is effectively proxying for the most relevant schooling inputs. Our estimates for the conditional wage gap when we use these two metrics alone in a Mincer model of wages are not substantially different from the baseline values reported below.

[23] It is also possible that cyclical fluctuations in overall school funding levels, which would be captured by age fixed effects, had an effect on labor market outcomes differently by race. If so, these effects will remain in the unexplained portion of the wage gap.

years.[24] As an example, an individual observed in the 1940 Census who is 25 years of age was a potential enrollee from the 1922/1923 school year through 1933/1934. For each individual, then, we assign a school quality measure which is the average of the school quality index in the county where he resided in 1935 over the years he could have been in school. Years for which there are missing data on school quality are excluded from both the numerator and denominator of Q_{ct}.[25] Therefore, the school quality metric varies across cohorts and counties and is best thought of as the typical quality of public education available to each respondent when they were ages 7 to 18.

The data linkage generates a base sample of 11,394 men aged 18-25 who report earnings, reside in 10 Southern states in 1940, report a discernible residence in 1935 for which school quality metrics are available, and report race of either "white" or "black" to the census enumerator. A critical issue for the empirical strategy described below is whether there is enough overlap in the school quality index of black and white respondents to justify a counterfactual exercise where blacks and whites were exposed to similar schools. Non-linearities in returns to education are included in the analysis, but a lack of common support across black and white school quality indices would hinder our ability to infer counterfactual black outcomes under a true "separate but equal" schooling system. Figure 1 compares kernel density estimates for black and white school quality indices and illustrates the distribution of black and white educational attainment. In both cases, there is considerable overlap, lending support to the empirical strategy described in the next section. Robustness checks described in the appendix show that restricting our analysis to the common support of these two human capital measures (effectively omitting the bottom half of the black subsample) modestly increases the conditional wage gap, which is consistent with greater degrees of discrimination among higher-skilled individuals.

[24] In practice, across-county variation in school quality measures are far more substantial than-within county differences across cohorts and attendance years, making the county of schooling assignment more important than the years of schooling one.

[25] An alternative approach is to assign school quality for years we infer individuals were actually in school. The issue is that age-in-grade distributions varied wildly so that individuals of a given age and highest grade attended cannot be credibly assigned to specific years in school.

4 Empirical Strategy and Results

Table 2 contains summary statistics of labor market outcomes, human capital investments, and other controls. Columns 1 and 2 of the table give average values for all men in the sample.[26] As noted before, a large number of men in the sample have no available income data and Columns 3 and 4 give the average value of these same characteristics for the sample used in the estimation. Due to the loss of non-wage agricultural workers in the baseline sample, occupational scores for this selected sample are slightly higher, as are measures of school quality and manufacturing value-added in their county of residence. In addition, the estimation sample is more urban than the underlying population. Section 4.1 presents baseline results for this working sample and Section 4.2 examines within-occupation wage gaps. In the Appendix, we estimate the impact of pre-market factors on employment *per se*, agricultural employment, and New Deal work relief employment, all of which can be estimated on a larger sample of individuals.

4.1 Baseline Results

Among the available labor market measures, it is clear that racial differences in labor force participation, employment rates, and average weeks worked from Table 2 are relatively small. Labor market wages, on the other hand, differ substantially by race. To evaluate the impact of human capital measures on these labor market outcomes, we estimate Equation 1 below with and without controls for educational attainment and school quality in the vector X_{icra}.[27] Differences in the black-white gap, δ, across these specifications reflect the ability of observable characteristics to account for racial differences in outcomes.

[26] The universe is all black and white men from the 1940 IPUMS sample aged 18 to 25 with a (discernible) county and state of residence in both 1935 and 1940 within our 10-state school quality region.

[27] We examine racial differences in returns to human capital, the β's below, in the Online Appendix. Black and white returns to human capital differ, but differential returns cannot be assessed with precision, and moreover, they explain a subjectively small proportion of the overall wage gap. A far greater proportion is attributable to differences in endowments and in the interaction between endowments and coefficients. Because of this, race-specific coefficients yield little change in simulated wage gaps under "separate but equal" counterfactuals (Section 7).

The estimating equation is:

$$lnY_{icra} = \alpha + \delta BLACK_i + \beta X_{icra} + \epsilon_{icra} \quad (1)$$

where Y_{icra} is the labor market outcome of interest for individual i educated in county c residing currently in county r of age a. In this setting, Y_{icra} measures one of four labor market outcomes: weekly wages, occupation score, annual wages, or weeks worked. $BLACK_i$ is a binary indicator, and the estimated wage gap is negative when black respondents have lower (conditional) labor market outcomes than whites.

When X_{icra} is excluded from the estimation of Equation 1, the parameter δ measures an unadjusted gap in earnings or occupational scores across races, or the difference in means across racial groups, as reported in Table 3. The unadjusted racial gap in weekly wages (Column 1) is 52.9 log points among the 11,394 individuals in the sample. The weekly wage gap reflects the combination of an unadjusted annual wage gap of 51.3 log points (Column 9) and a weeks worked gap of 1.6 (log) weeks favoring blacks (Column 13), leading to a larger weekly than annual wage gap. The two groups have an unconditional occupational score gap of 35.8 log points (Column 5).[28]

We then calculate a conditional wage gap, first conditioning only on age fixed effects and characteristics of the individual's county of residence which may have impacted wage levels. These controls include those listed in Table 2: urbanicity of the county of residence (to proxy for cost of living differences) as well as per capita manufacturing value, per capita retail sales and per capita crop value to represent average productivity in manufacturing, services, and agriculture, respectively. These controls lower wage and occupational score gaps, albeit very slightly, while raising the gap in weeks worked (Columns 2, 6, 10, and 14 of Table 3).

Finally, we include third-degree polynomial functions of educational attainment and school quality variables in X_{icra} to capture non-linearities in the impact of these variables on labor market outcomes. Identification of δ comes from within cohorts, relying on in-

[28]We limit the occupational score analysis to those who also report wages to keep samples consistent, although they differ somewhat due to individuals who do not report an occupation, but do report wages. We relax this constraint in the appendix robustness tests.

dividual variation in years of schooling and variation in school quality across counties and races, conditioned on age fixed effects and local economic circumstances that would have affected average wages. Our preferred model does not include county fixed effects, since we expect the intensity of wage discrimination to have varied spatially (Sundstrom, 2007),[29] although as we show in the appendix, the conditional wage gap is broadly robust to the inclusion of county fixed effects.

The addition of these human capital controls dramatically reduces the racial gap in labor market outcomes. Column 3 of Table 3 indicates that after controlling for human capital, the racial gap in weekly wages falls to 15.9 log points, 70 percent lower than the unadjusted gap. The conditional gap in weeks worked (Column 15) significantly favors black men, such that the conditional annual wage gap (Column 11) is 82 percent lower than the unconditional gap. Though human capital may have narrowed wage gaps, their closure was inhibited by occupational sorting. Results for occupation score (Column 7) indicate that human capital differences explained blacks' occupational standing relative to that of whites, but at a smaller rate (55 percent) than relative wages.

School quality and individual attainment are highly correlated, and it is not clear from δ estimates alone which measure of human capital is primarily responsible for attenuating the black-white earnings gap. Adding covariates sequentially is one approach to disentangling the contribution of school quality from that of attainment. But Gelbach (forthcoming) shows that this procedure can lead to misleading results, and that such decompositions depend on the order in which controls are added to the model. We use Gelbach's decomposition framework to estimate the relative contribution of years of schooling and school quality to wages and occupational scores.[30] In Table 3, the contribution of each term (in log points) is displayed beneath the δ coefficient in Columns 3, 7, 11, and 15. Differences

[29]Suppose that each county has a different level of labor market discrimination. A county fixed effect is an unobserved factor that affects average wages in that county, which will include the discrimination value for that county.

[30]Gelbach (forthcoming)'s procedure stems from the identity $\hat{\beta}_1^{base} = \hat{\beta}_1^{full}(X_1'X_1)^{-1}X_1'X_2\hat{\beta}_2$, where $\hat{\beta}_1^{base}$ ($\hat{\beta}_1^{full}$) is a vector of X_1 coefficients in the limited (fully conditioned) model and $\hat{\beta}_2$ is a vector of X_2 coefficients. In our context, X_1 is a race indicator, and X_2 factors are years of schooling and school quality, two contributing factors to race-based differences in labor outcomes.

in educational attainment account for 17.2 log points of the attenuation of the black-white gap between Columns 2 and 3, while school quality accounted for a somewhat smaller 16.5 log points. Similar results obtain for annual wages. In contrast, educational attainment accounts for the majority of attenuation in the occupational score gap.

The primary threat to the internal validity of results is that of a classic omitted variable, correlated with both human capital and earnings in a way that falsely attributes labor market gaps to human capital or (implicitly) to discrimination. In our view, the primary concern on this dimension is that school quality measures embodied in X_{icra} in Equation 1 are proxies for county race relations in general. If so, it is not at all surprising that including school quality in a wage equation goes a long way towards explaining the racial wage gap. Black Southerners could have experienced discrimination in both the labor market and in decisions that affected school quality. If those two factors are highly correlated, the human capital inputs available to blacks may simply be proxies for overall relations and Table 3 results understate the role of labor market discrimination in explaining the earnings gap and overstate the role of school quality provision.

In recognition of this concern, our preferred specification includes both human capital controls as well as a full interaction between the cubic in school quality and the cubic in educational attainment. This allows us to assess the relative contributions of school quality on its own and school quality as it serves to enhance educational attainment, the former being more indicative of omitted discrimination in the presence of the latter.[31] These results are located in Columns 4, 8, 12 and 16. In each case, the conditional gap is little changed and the isolated contribution of school quality toward changing the unconditional wage gap is small, imprecise, and/or positive. The majority of the school quality effect for weekly and annual wages comes through its interaction with educational attainment.[32] Because this specification produces larger wage gaps than that with human capital controls alone

[31] We further address the underlying identification threat by limiting our analysis to migrant blacks whose county of residence was not their county of schooling (Section 5), by conditioning on imputed AGCT scores (Section 6), by conditioning on fixed effects for county of residence or county of schooling (Appendix), and by examining an Oaxaca decomposition of the black-white gap (Appendix).

[32] This is also consistent with work by Card & Krueger (1992a), who find that higher school quality, measured at the state level, is associated with enhanced returns to schooling.

(uninteracted), it will be our baseline for the remainder of the paper.

From this preferred specification we conclude that differences in human capital measures account for the majority of the black-white wage and occupational score gaps in 1940. The remaining coefficient on δ, 17.1 log points for weekly wages and 11.3 log points for annual wages, represent 68 and 78 percent reductions, respectively, from the unconditional wage gap. To put these residual differences in perspective, note that 11.3 log points aligns with the modal conditional wage gap estimate listed in Table 1, and that 17.1 log points lies at the higher end of the range of these post-Civil Rights estimates. In the next section, we examine within-occupation wage gaps to further delve into the role of discrimination conditional on occupation sorting.

4.2 Within-Occupation Wage Gap Results

The fact that human capital explains relatively less of the occupational standing gap than wage gaps begs the question of whether occupational sorting itself was a prominent driver of the black-white wage gap (Higgs, 1977).[33]

Figure 2 plots the distribution of black and white workers across nine broad occupation categories,[34] along with average log annual wages for each category. There are substantial differences in the distribution across occupation categories, and the wage measures layered on the histogram indicate, unsurprisingly, that wages are higher in the occupations where whites are disproportionately represented.

In a regression framework, we calculate the conditional within-occupation racial gap in weekly and annual wages and in weeks worked by introducing fixed effects for nine occupation categories to Equation 1.[35] We continue to condition on county covariates, cubics in educational attainment and school quality, and the interaction of school quality with attainment measures. Estimates of δ in Table 4 show that the within-occupation weekly wage gap is reduced to 12.8 log points and that the annual gap is an insignificant 4.1 log

[33] According to Higgs (1977), racially dependent sorting across high-wage and low-wage firms is another likely source of the overall black-white gap. We do not observe respondents' employers in the 1940 census.

[34] The categories are professionals, managers/officials/proprietors, clerical and kindred, sales workers, craftsmen, operatives, service workers, agricultural laborers, and general laborers.

[35] Results do not differ in any meaningful way if we use 3-digit code fixed effects.

points. The difference between Columns 3 and 6 is reconciled by the conditional gap in weeks worked, which expands with controls for occupation fixed effects. Within occupations, black and white men of equivalent schooling had similar annual earnings, but given that blacks worked 8.7 percent more over the course of the year, the weekly earnings gap remains significant at 12.8 percent. These findings support the idea that discrimination manifested in part via occupational sorting, but not so much as to eliminate the within-occupation correlation between race and wages.

5 Collinearity in Public and Labor Market Discrimination

As discussed in Section 4.1, one major impediment to interpreting conditional wage gaps as wage discrimination is the possibility that local wage discrimination is collinear with local school quality. If so, then the measured contribution of school quality to the overall wage gap may be misattributed. As a first step in assuring this attribution is appropriate, we showed in Section 4.1 that school quality impacts wages through primarily through its interaction with years of schooling as would be expected if school quality was not simply proxying for wage discrimination. In this section, we take additional steps to ensure the same. Because decisions about local school quality may have reflected local discriminatory tastes, we exploit the existence of inter-county migrants in our data - those who were likely educated in counties other than their county of residence in 1940. Focusing on these black migrants breaks the collinearity between school quality and labor market discrimination in each individual's 1940 county of residence (although it adds the question of selection into migration). If human capital measures are less successful at explaining the earnings gap of new resident blacks relative to all resident whites (i.e., if the conditional gap is much larger between these two groups than between blacks and whites more broadly), then we would suspect that estimates in Table 3 are driven by local race relations more so than local school quality.

We omit from our sample all black individuals living and working in the same county in which they were educated, as evidenced by their 1935 location. Practically, this involves eliminating 90% of the black sample so that identification comes from a relatively small number of blacks. Migrating blacks had somewhat higher wages than their non-migrating peers, and this generates a lower baseline wage gap between blacks and whites as evidenced in Columns 1 (weekly wage) and 7 (annual wage) of Table 5, although the occupation score gap is unchanged.[36]

The remainder of Table 5 repeats the analysis reported in Table 3 with non-migrating blacks excluded. Contrary to the expected results if collinearity between school quality and labor market discrimination was driving our estimates, Column 3 indicates that human capital controls result in a nearly equivalent conditional weekly wage gap (17.8 versus 17.1 log points). The conditional black-white gap in annual wages falls to 3.8 log points (Column 9) and is insignificantly different from zero.

We do find some evidence that the occupation score gap grows after eliminating non-migrant blacks, from 16.0 in Table 3 to 28.5 log points in Table 5 Column 6. The implication is that the baseline gap closure we observed for this outcome – with variance limited by occupational sorting – may have been driven by unobserved variables more so than human capital.

Still, we take the wage results in Columns 3 and 9 of Table 5 to indicate that school quality measures are not simply serving as a proxy for local race relations in determining pay. The fact that human capital controls result in a lower annual pay gap when we limit the black population to inter-county migrants is itself an interesting conclusion, with the caveat that migrating blacks are very small in number and perhaps positively selected, even after controlling for observable human capital.

6 Adding Proxies for Unobserved Ability

A common concern in the discrimination literature is the notion of an unobserved ability gap that would manifest as a wage gap. If average ability, conditional on observable human

[36]Summary statistics for the migrating sample are available in the Appendix, Table 7.

capital, differs across blacks and whites, the conditional wage gap does not accurately reflect the depth of labor market discrimination. The direction of the bias is ambiguous. In modern data, Lang & Manove (2011) show that controls for AFQT scores alongside years of schooling increase estimates of the pay gap because blacks tend to have more years of schooling for a given AFQT-measured ability. Omitting ability measures – as we do in Equation 1 – may therefore understate the conditional pay gap. In our context, it may well have been the case that only the highest ability black students would have achieved higher levels of educational attainment given the pervasive impediments to attendance. On the other hand, it may be that blacks were less productive than observationally equivalent white counterparts due to differences in the intergenerational transmission of human capital or other pre-market investments outside of schooling. If so, we understate human capital and overstate the potential role of labor market discrimination in Table 3.

One potential proxy for unobserved ability is parental education. Lang & Manove (2011) find that family background controls are important parts of the wage model using NLSY data. In the 1940 census, however, this information is only available for respondents who were still living with their parents (57 percent of the analytical sample of 18 to 25-year old males[37]). Adding controls for this incomplete measure of family background along with an indicator for missing parental education modestly widens the conditional wage gap, as shown in the second column of Table 6. The conditional weekly wage gap rises from 17.1 log points to 18.0 log points, and the conditional annual wage gap rises from 11.3 log points to 13.3 log points.

A second proxy for unobserved ability used in the modern literature is performance on a standardized exam. Several studies examining features of the wage gap in the NLSY panel utilize AFQT scores as pre-market proxies for ability (See Table 1 for examples). Through a historical fluke, standardized test scores are available for a subset of World War II enlistees for several weeks in 1943.[38] Unfortunately, directly linking our 1940 IPUMS sample of

[37]A separate issue is the possibility that parents misreported their grown children's educational attainment as years rather than grades completed. To the extent that this was more prevalent among black respondents than white, it will serve to increase the wage gap by overstating black men's education.

[38]For a limited time, WWII enlistment cards contained AGCT scores in place of weight. We know of no

males to the WWII enlistment data for this window of time, matching on name and county of residence, generates too small a sample for meaningful analysis. Instead, we utilize the fact that WWII records include a measure of highest grade completed, much like the 1940 census sample, as well as an exact county and state of residence at enlistment to assign human capital measures to each individual in the WWII records where AGCT is recorded. For these WWII enlistees, then, we have a measure of race, educational attainment, school quality, and estimated ability.[39]

We can use this sample to impute AGCT scores for men in the 1940 census conditioning on educational attainment, school quality, and race. Although these are imputed measures, they do answer the fundamental omitted variable question: conditional on observable human capital, is there a correlation between race and unobserved ability? We wish to remain as agnostic as possible about the relationship between race, school quality, attainment, and AGCT scores, recognizing that black/white differences in AGCT conditional on other human capital metrics may differ across the distribution of those metrics. We limit the age range of WWII enlistees to mirror that of our census sample.[40] We then exploit the fact that race and educational attainment are binary and categorical variables, respectively, and use two different methods to control for school quality, a continuous variable.

- Method 1: We subdivide school quality into 10 deciles and calculate average AGCT scores within each race/educational attainment/school quality decile bin. We assign an imputed AGCT score to each individual in the baseline sample accordingly, with

evidence that this test was racially unbiased. Enlistment in the armed forces, however, was conditional on a minimum literacy standard so that the test results would not have been racially biased for literacy reasons. Like the modern AGCT, the test appears to measure acquired ability rather than inherent cognition (Zeidner & Drucker, 1983). See Troesken et al. (2012) for additional details.

[39]The assumption that allows us to link school quality to individuals is that their county of residence at enlistment is the county where they were educated. This is a worse assumption than that employed in the main analysis due to migration between schooling and enlistment.

[40]Neal & Johnson (1996) are careful to limit the AGCT scores in their analysis to those taken prior to entrance in the labor market arguing that "[j]ob experience and post-secondary education surely enhance human capital and will therefore increase test scores. If discrimination limits access to these human capital investments, then postentry discrimination contaminates the test scores (p.873)." Because the AGCT test was reported at enlistment, the youngest age at which we observe this score is age 18 with a large mass of observations at age 19. Seventy-five percent of the individuals in the WWII enlistment records used for ability imputation are 20 and younger.

the restriction that bins with fewer than 20 observations in the WWII data are omitted.

- Method 2: We specify that AGCT is a function of a 5th-order polynomial in school quality within each race/educational attainment bin. We then use the parameter estimates to impute AGCT for the baseline sample.

With these two methods for imputing AGCT scores, we revisit Equation 1 with imputed AGCT as an additional control variable. The additional information in these specifications is the estimated relationship between observed schooling, school quality, and AGCT-measured ability. Results for both weekly and annual wages are located in Table 6. Controlling for estimated ability, the black-white wage gap falls substantially. The weekly wage gap falls to between 10.3 (Column 3) and 11.9 log points (Column 4), depending on the imputation method. For annual wages, the remaining gaps in Columns 8 and 9 are insignificantly different from zero.

As a falsification exercise, we repeat the second imputation method for WWII enlistees' weight in the same months of 1942, when the weight field should have contained physical weight and not AGCT scores. That is, we estimate enlistees' weight as a function of the school quality polynomial by race/attainment bin, and then map parameter estimates to the 1940 census sample, "predicting" respondents' weight. Results are found in Columns 5 and 10 of Table 6. The conditional wage gap rises slightly from the baseline to 18.1 log points and the annual wage gap to 11.9. The contrasts between Columns 4 and 5 and between Columns 9 and 10 are telling: estimating ability by its known relationship to observables adds important information to the model, whereas adding a spuriously constructed "weight" has little bearing on the wage gap.[41]

A final caveat on the imputation of AGCT scores is that the sample of individuals in the 1943 enlistment records may be selected in some way. Selection would impact the estimate of δ only if selection differed across blacks and whites. We do observe higher educational attainment in the WWII enlistment records for both blacks and whites, perhaps due to the

[41] In addition, weight is insignificantly predictive of wages in the estimation for 1942 but predicted AGCT is positively, significantly correlated in 1943, further indication that the 1943 "weight" data do measure ability.

literacy restrictions on enlistment. The difference in (log) average educational attainment between the enlistment records and the census records is larger for blacks than whites, indicating more positive selection on observables amongst blacks. Educational attainment is explicitly accounted for in the imputation, but if the same selection pattern is true for AGCT scores as well, then we overestimate the ability of blacks in the census sample and our estimates of δ in Table 6 are biased away from zero.

7 Counterfactual Estimates

The implicit counterfactual exercise in our baseline results calculates the wage gap in 1940 wages if blacks had achieved the same level of education in schools of comparable quality to whites. Columns 4 and 12 of Table 3 indicate that the remaining gap would have been roughly 17.1 log points of weekly wages and 11.3 log points of annual wages.

But this inherent counterfactual ignores historical realities. First, education budgets differed substantially across counties, as did the geographic distribution of blacks and whites across the South. Assigning all blacks the average education quality of whites presumes perfect mobility of households and education funds, when in reality, education funding was highly decentralized at the county or sub-county level. Second, there were many reasons for differing educational attainment by race, only some of which were within the purview of policy makers. Finally, it is possible that the *returns* to education differed by race so that equalizing endowments without equalizing yields would have generated more muted wage impacts.

To address these issues, we present a series of counterfactual estimates in Table 7. Means and unconditional gaps are located in Panel 1. We allow for a balanced budget constraint within counties and equalize black and white school quality at the county average (weighted by black and white school enrollment). We then predict wage outcomes using a restricted version of Equation 1 where returns to education are not allowed to vary by the quality of schooling.[42] These results are located in Panel 2. Results in Row 2A indicate

[42]Our preferred specification of Equation 1 conditions on third-degree polynomial functions of attainment, school quality, and full interactions therein. This model describes the wage-setting process well (as we

that counterfactual racial wage gaps would have fallen to between 35 and 38 log points, a reduction of 28 to 31 percent relative to the unadjusted baseline. Note here that although the binding budget constraint effectively lowers the school quality experienced by whites, very modest changes in white earnings between Panels 1 and 2 are indicative of diminishing returns to school quality.

In Row 2B we allow for race-specific returns to human capital in constructing the separate but equal counterfactual. Foreshadowing decomposition analyses in the Appendix, we show here that this more flexible model registers almost no change in the weekly wage gap and lowers the estimated annual gap by 6 log points.[43]

As a second step to estimate impacts of separate but equal, we allow educational attainment to be endogenous, recognizing that time in school is a function of school quality (Margo, 1987), and that equalized school resources would have affected earnings through the years of schooling channel as well as the input quality channel. Of course, these changes, in turn, likely affected cognitive ability, but we do not seek to model this pathway here.

We rely on quasi-experimental evidence of the effect of school quality from other work. Aaronson & Mazumder (2011) estimate the effect of exposure to Rosenwald schools – in terms of classroom capacity per black school-aged youth – on individual years of schooling, among other outcomes. We convert their Rosenwald exposure measure into a change in our calculated Z-score, and then convert the elasticity of educational attainment with respect to Rosenwald schools to an elasticity per unit of school quality in our sample. We then use this elasticity to calculate the counterfactual level of educational attainment after a "separate but equal" mandate. On average, estimated attainment increases by 1.19 years for blacks and falls by 0.43 years for whites. [44] Results are presented in Panel 3 of Table

demonstrate in Section 4.1) but does not perform consistently when we extrapolate black earnings under substantially higher school quality. Therefore, we prefer a restricted model – without attainment-quality interactions – for this projection, reminding the reader that the residual wage gap is little changed between the restricted and fully interacted specification (Table 3).

[43] To implement race-specific returns to human capital, we re-estimate Equation 1 with interactions between human capital variables and a race indicator. We then equalize school quality, predict counterfactual wages, and report the levels and racial difference.

[44] Aaronson & Mazumder (2011) express the effect of school quality on years of schooling with respect

7. With endogenous years of schooling and race-neutral coefficients, the wage gap falls to between 26 and 30 log points (Row 3A), a reduction relative to the baseline of between 39 and 52 percent. In Row 3b, we again allow for race-specific returns to education quality and attainment and, again, this distinction matters little for our result as estimates differ by 1 to 3 log points.

Overall, we conclude that a separate but equal mandate would have reduced labor market inequalities substantially, reducing the unconditional weekly wage gap by up to 44 percent and the annual wage gap by up to 52 percent.

8 Conclusion

Recent labor market studies have highlighted the importance of human capital in explaining the black-white wage gap. We ask the same question for 1940 workers - how far can human capital disparities go in explaining the large pre-war racial wage disparity? Incorporating new data on race-specific school quality in ten southern states, we document a predominant role of school quality and educational attainment in determining wage inequality, with the potential scope of discrimination limited to 11 - 17 log points. Once we control for estimated AGCT scores imputed from WWII enlistment records, the conditional gap falls even more, to 0 - 11 log points. These estimates of conditional wage gaps are not dissimilar from levels observed today.

The power of education to drive labor market wages is echoed in a counterfactual exercise whereby school quality is equalized across races in the South. Under this "separate but equal" standard, we estimate a counterfactual weekly wage gap of between 30 and 39 percent, a fraction of the 53 percent gap observed in 1940. Education equality would have

to their quality measure "Rosenwald exposure," that is, the number of classrooms per 45 rural blacks aged 7 to 17 in a county. We lack access to the same population measures, but we substitute blacks aged 10 to 20 in a county, multiplied by the percent of the overall county population that is rural. Because classrooms are not one of our school quality metrics, we make the innocuous assumption that each classroom represented an additional teacher (as supported by the historical record) and convert the change from 0 to 1 in Rosenwald exposure to the number of additional black teachers in a county, based on our rural black population estimates. We then re-calculate the teachers-per-student Z-score in each county after Rosenwald exposure changes from 0 to 1. We divide the Aaronson & Mazumder (2011) reported elasticity by this change in Z-score to get the average change in years of schooling per Z-score unit and use this as our quasi-experimental elasticity.

been a powerful tool for raising black economic standing in the South, and the lost opportunity of enforcing separate but equal in southern states, by our estimates, reduced the earnings capacity of this generation of Southerners by up to 52 percent. Spillover benefits from black gains of this magnitude are unfathomable, but likely would have touched migration (Margo, 1991), black-white wage convergence outside of the South (Boustan, 2007), health, and subsequent generations.

Although we must be careful in labeling residual wage gaps as discrimination, the absence of large conditional gaps seems incompatible with what we know about the Jim Crow South. Black Southerners were excluded from civil life through a variety of measures that effectively eviscerated their participation in the political process. One result was a denial of the provision of black education at the same level as that provided to whites and an enormous roadblock to the accumulation of human capital. Yet, as we show, blacks participated in economic life, exhibiting labor force participation and employment rates not dissimilar from those of whites and (conditional) earnings ratios not remarkably different from blacks later in the century. Employers of 1940 may have held animus or equality aversion toward black individuals, but the effect of these attitudes on black wages would have been offset to some degree by profit-maximizing objectives. These profit-maximizing values, not necessarily shared by largely white voting constituencies, explain why severe racial discrimination in the provision of public goods could coincide with a more equal (conditional) labor market. We also note that our data do not allow us to observe racial sorting at the firm level, which may also have facilitated "equal pay for equal human capital".

Though the Civil Rights era engendered a marked disruption in labor market discrimination (Heckman & Payner, 1989; Donohue & Heckman, 1991), it did so against a backdrop of converging school quality gaps. If conditional wage gaps were perhaps 35 percent smaller in 2000 than in 1940,[45] this calls into question interpretations of 20^{th} century black relative wage growth as a manifestation of a decline in labor market discrimination alone. It would seem, rather, that the narrowing wage gap that characterized the late 20^{th} century is attributable in large part to gains in the quality of blacks' pre-market human capital, which

[45] This compares the modern literature's modal 11 log point gap to our 17 log point result for weekly wages.

in turn is predominantly attributable to improvements in the quality of public schooling available to black youths.

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Tables and Figures

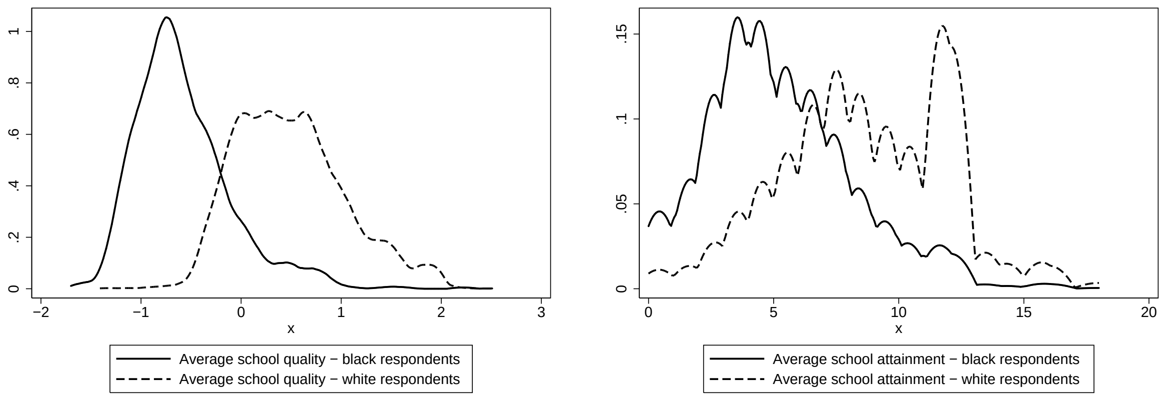

FIGURE 1: School quality and educational attainment kernel densities for black and white 1940 census respondents

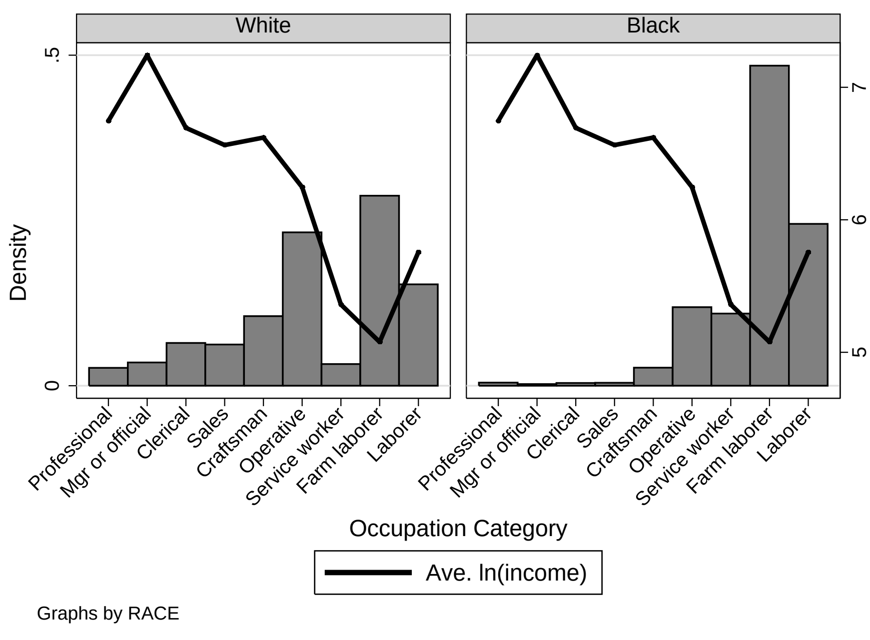

FIGURE 2: Occupation distributions for black and white 1940 census respondents. Average (log) wages within those occupations, across both races, are reported on the second axis.

TABLE 1: Decomposition of the Black-White Pay Gap: The Role of Human Capital

Study	Data	Proxy for human capital	Percentage of total wage gap explained by human capital	Log gap remaining after controlling for human capital
Altonji & Blank (1999)	1980 CPS	years of schooling	25%	-0.11
Altonji & Blank (1999)	1996 CPS	years of schooling	43%	-0.12
Altonji & Blank (1999)	NLSY1979	years of schooling, AFQT	61%	-0.06
Altonji & Pierret (2001)	NLSY1979	years of schooling, AFQT, father's education, labor force experience	all	*insig.*
Card & Krueger (1992b)	1960-1980 Census	years of schooling, state-level school quality	20% of gap narrowing	*n/a*
Carneiro et al. (2005)	NLSY; men 26-28	years of schooling, 8^{th}-grade equivalent AFQT	35-50%	-0.133 - -0.241
Fryer (2011)	NLSY1979	AFQT	women: all, plus men: 72%	women: 0.127 men: -0.109
Fryer (2011)	NLSY1997	AFQT	women: 71% men: 39%	women: -0.044 men: -0.109
Fryer (2011)	College and Beyond 1976	SAT	women: 53% men: 44%	0.286 for women -0.152 for men
Lang & Manove (2011)	NLSY1979	AFQT, years of schooling, school inputs	70%	-0.11
Neal & Johnson (1996)	NLSY1979	AFQT	100% men: 70%	women: *insig.* men: -0.072
Oaxaca & Ransom (1994)	1988 CPS	years of schooling	43%	-0.125
O'Neill et al. (2006)	NLSY1979	AFQT, father's education, non-cognitive skills	46-114%, rising with wage quantile	falls with quantile

TABLE 2: Summary Statistics

	(1) ALL BLACK	(2) ALL WHITE	(3) BASELINE SAMPLE BLACK	(4) BASELINE SAMPLE WHITE
Individual				
Average Annual Wage Income *in natural log*	5.42	5.93	5.42	5.92
Average Weekly Wage *in natural log*	1.87	2.40	1.87	2.39
% Reporting	58.8	56.0	100.0	100.0
Occupational Score *in natural log*	6.99	7.35	6.99	7.35
% Reporting	87.0	82.2	97.5	96.4
Average Weeks Worked	40.9	40.8	39.0	38.7
Unemployment Rate at Time of Census	9.17	9.52	9.90	8.92
Duration of Unemployment	38.8	45.4	35.4	43.8
Labor Force Participation Rate	88.9	85.0	98.2	97.5
Highest Grade Completed	5.6	8.9	5.6	8.9
School Quality Index (Standardized (0,1))	-0.55	0.47	-0.50	0.55
State of Residence in 1940				
Alabama	12.8	9.0	12.3	8.7
Arkansas	5.6	6.5	4.0	6.0
Georgia	15.5	9.6	18.1	9.7
Kentucky	2.7	12.2	3.0	10.9
Louisiana	11.4	7.5	12.4	7.5
Mississippi	6.6	2.2	3.8	2.2
North Carolina	14.3	12.6	14.3	12.7
South Carolina	13.9	5.6	14.0	6.2
Tennessee	5.8	11.6	6.4	11.0
Texas	11.6	23.3	11.8	25.0
County of Residence				
Percent Rural	70.4	69.2	66.2	64.2
Per Capita Manufacturing Value	111.2	111.9	126.6	127.2
Per Capita Retail Sales	0.22	0.24	0.24	0.26
Per Capita Crop Value	87.5	84.0	78.4	75.5
Number of observations	5,423	14,849	3,141	8,253

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. Includes all black and white males from the 1940 IPUMS sample aged 18 to 25 who lived within the 10 Southern states covered by our school quality data 1935 with reported years of schooling and school quality. Columns 3 and 4 contain only those individuals for whom earnings data are available.

TABLE 3: Estimates of Black-White Labor Market Outcome Gaps

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	ln(Weekly Wage)				ln(Occupation Score)			
BLACK-WHITE GAP	-0.529	-0.496	-0.159	-0.171	-0.358	-0.348	-0.160	-0.168
	(0.024)	(0.023)	(0.035)	(0.036)	(0.016)	(0.014)	(0.024)	(0.025)
Contribution of	–	–	-0.165	-0.028	–	–	-0.055	0.111
School Quality	–	–	(0.026)	(0.047)	–	–	(0.018)	(0.040)
Contribution of	–	–	-0.172	-0.165	–	–	-0.133	-0.130
Educ Attainment	–	–	(0.010)	(0.011)	–	–	(0.007)	(0.008)
Contribution of	–	–	–	-0.132	–	–	–	-0.161
Interaction	–	–	–	(0.044)	–	–	–	(0.039)
County Covariates?		✓	✓	✓		✓	✓	✓
Age Fixed Effects?		✓	✓	✓		✓	✓	✓
Human Capital Controls?			✓	✓			✓	✓
Interacted HC Controls?				✓				✓
N	11,394	11,394	11,394	11,394	11,021	11,021	11,021	11,021
Adjusted R-Squared	0.10	0.22	0.29	0.29	0.10	0.17	0.25	0.25

Column	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Outcome	ln(Annual Wages)				ln(Weeks Worked)			
BLACK-WHITE GAP	-0.513	-0.477	-0.090	-0.113	0.016	0.019	0.069	0.058
	(0.027)	(0.025)	(0.041)	(0.041)	(0.014)	(0.014)	(0.024)	(0.024)
Contribution of	–	–	-0.186	0.054	–	–	-0.020	-0.023
School Quality	–	–	(0.029)	(0.051)	–	–	(0.018)	(0.010)
Contribution of	–	–	-0.202	-0.188	–	–	-0.030	0.083
Educ Attainment	–	–	(0.013)	(0.014)	–	–	(0.007)	(0.043)
Contribution of	–	–	–	-0.231	–	–	–	-0.099
Interaction	–	–	–	(0.046)	–	–	–	(0.040)
County Covariates?		✓	✓	✓		✓	✓	✓
Age Fixed Effects?		✓	✓	✓		✓	✓	✓
Human Capital Controls?			✓	✓			✓	✓
Interacted HC Controls?				✓				✓
N	11,394	11,394	11,394	11,394	11,394	11,394	11,394	11,394
Adjusted R-Squared	0.06	0.21	0.27	0.27	0.00	0.05	0.05	0.06

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. Columns 1, 5, 9, and 13 represent coefficients from an unadjusted regression of log weekly wages, log occupational score, log annual wage, and log weeks worked, respectively, on race alone. Columns 2, 6, 10, and 14 include age fixed effects and county covariates in the regression. Columns 3, 7, 11 and 15 include cubic functions of educational attainment and school quality as per Equation 1. Columns 4, 8, 12, and 16 include controls for cubic functions of human capital and their complete interaction. We follow Gelbach (forthcoming) to estimate the separate contributions of school quality and years of schooling in attenuation of the black-white gap (i.e., the difference between the age and location-adjusted wage gap in columns 2, 6, 10 and 14 and the human capital-adjusted model in columns 3-4, 7-8, 11-12, and 15-16). County covariates include the percent urban population, crop value per capita, retail sales per capita, and manufacturing value added per capita.

TABLE 4: Estimates of Black-White Labor Market Outcome Gaps, With Occupation Fixed Effects

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome	ln(Weekly Wage)			ln(Annual Wages)			ln(Weeks Worked)		
BLACK-WHITE GAP	-0.280	-0.287	-0.128	-0.207	-0.218	-0.041	0.073	0.069	0.087
	(0.019)	(0.019)	(0.031)	(0.024)	(0.023)	(0.037)	(0.015)	(0.015)	(0.025)
Occupation Category Fixed Effects?	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Covariates?		✓	✓		✓	✓		✓	✓
Age Fixed Effects?		✓	✓		✓	✓		✓	✓
Interacted HC Controls?			✓			✓			✓
N	11,021	11,021	11,021	11,021	11,021	11,021	11,021	11,021	11,021
Adjusted R-Squared	0.33	0.36	0.38	0.26	0.32	0.35	0.03	0.06	0.07

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010) and annual reports of state education departments. See discussion in Section 4.2

TABLE 5: Estimates of Black-White Labor Market Outcome Gaps, Excluding Non-Migrant Blacks

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome	ln(Weekly Wage)			ln(Occupation Score)			ln(Annual Wages)			ln(Weeks Worked)		
BLACK-WHITE GAP	-0.411	-0.443	-0.178	-0.360	-0.372	-0.285	-0.415	-0.455	-0.038	-0.004	-0.012	0.140
	(0.048)	(0.055)	(0.072)	(0.038)	(0.036)	(0.050)	(0.065)	(0.072)	(0.097)	(0.039)	(0.039)	(0.057)
County Covariates?		✓	✓		✓	✓		✓	✓		✓	✓
Age Fixed Effects?		✓	✓		✓	✓		✓	✓		✓	✓
Interacted HC Controls?			✓			✓			✓			✓
N	8,464	8,464	8,464	8,166	8,166	8,166	8,464	8,464	8,464	8,464	8,464	8,464
Adjusted R-Squared	0.01	0.16	0.25	0.01	0.10	0.22	0.05	0.18	0.27	0.00	0.06	0.07

Notes: See notes to Table 3. Black males who *did not* migrate between counties over 1935-1940 are excluded from the estimation. See further discussion in Section 5.

TABLE 6: Estimates of Black-White Labor Market Outcome Gaps, Including Unobserved Ability Estimates

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome			ln(Weekly Wage)					ln(Annual Wages)		
BLACK-WHITE GAP	-0.171	-0.180	-0.103	-0.119	-0.181	-0.113	0.133	0.012	-0.017	-0.119
	(0.036)	(0.036)	(0.048)	(0.048)	(0.037)	(0.041)	(0.041)	(0.055)	(0.052)	(0.040)
Baseline	✓					✓				
Parental education controls		✓					✓			
Cognitive ability imputation method 1			✓					✓		
Cognitive ability imputation method 2				✓					✓	
Cognitive ability imputation falsification					✓					✓
N	11,394	11,394	11,261	11,394	11,394	11,394	11,394	11,261	11,394	11,394
Adjusted R-Squared	0.29	0.30	0.30	0.29	0.29	0.27	0.28	0.27	0.27	0.27

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010), World War II enlistment records, and annual reports of state education departments. See specifications and further discussion in Section 5.

TABLE 7: Counterfactual Estimates of Black and White Earnings

Column	(1)	(2)	(3)	(4)
Outcome	ln(Weekly Wage)		ln(Annual Wage)	
	Black	White	Black	White
(1) As measured in 1940				
	1.865	2.389	5.408	5.915
Black-White Gap	-0.529		-0.513	
	(0.024)		(0.025)	
(2) Counterfactual "separate but equal" estimates				
(2a) With pooled coefficients	1.979	2.361	5.521	5.875
Black-White Gap	-0.382		-0.354	
	(0.012)		(0.015)	
(2b) With race-specific coefficients	1.979	2.362	5.574	5.868
Black-White Gap	-0.385		-0.298	
	(0.012)		(0.020)	
(3) Counterfactual "separate but equal" estimates with endogenous attainment				
(3a) With pooled coefficients	2.036	2.334	5.588	5.846
Black-White Gap	-0.298		-0.257	
	(0.013)		(0.016)	
(3b) With race-specific coefficients	2.009	2.332	5.593	5.833
Black-White Gap	-0.325		-0.244	
	(0.012)		(0.020)	

Notes: Authors' Calculations from 1940 IPUMS data (Ruggles et al., 2010), annual reports of state education departments, and Aaronson & Mazumder (2011) results for the quasi-experimental impact of school quality on years of schooling. The table compares black and white weekly and annual wages under counterfactual levels of school quality and educational attainment. Counterfactuals are estimated by estimating Equation 1, with controls for individual and county covariates, and then altering covariates of interest, and predicting outcomes. The remaining differences are estimated as the coefficient on RACE in an equation with the predicted values as the dependent variable and no other controls. For each outcome, row (2a) lists the counterfactual values under equalized school quality for black and white students at the county average, holding years of schooling constant. Row (2b) equalizes school quality, but also allows for race interactions in school quality coefficients in the prediction equation. Row (3a) allows black years of schooling to increase with school quality according to the elasticity of time in school with respect to school quality as reported by Aaronson & Mazumder (2011), Table 5, column 1 (1.186 years per Rosenwald exposure). Row (3b) again allows for race-specific returns to attainment and school quality.